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for the late Professor Clarence H. Graham

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A final report NR 197-009 listing personnel and publications for three years and describing research carried out during the final year in the areas of visual perception (Ames window, perceptual grouping; illusions), movement perception, retinal integration and differentiation.

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REPORT OF INVENTIONS AND SUBCONTRACTS
(Pursuant to "Patent Rights" Contract Clause)

Form Approved
Budget Bureau No. 22-R160

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This form *(in triplicate)* is for use in submitting INTERIM and FINAL reports to the Contracting Officer.

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A FINAL report shall be submitted as soon as practicable aft the work under the contract is complete and shall include (a) summary of all inventions required by the contract to l reported, including all inventions previously reported and ar inventions since the last INTERIM report; and (b) any require information for subcontracts which has not previously bee reported.

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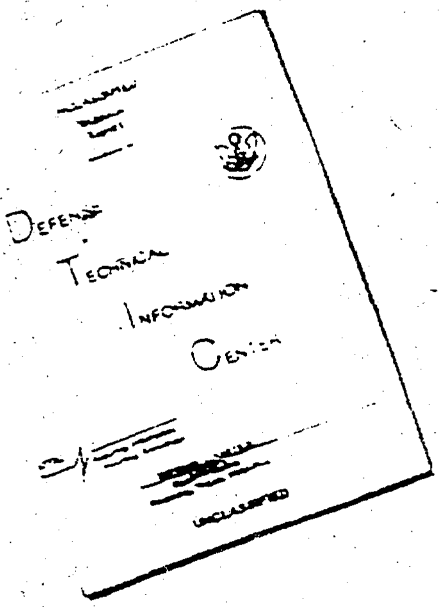

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N00014-67-A-0108-0009

FINAL REPORT

August 30, 1971

To: Chief of Naval Research, Arlington, Virginia 22217
Attention: Engineering Psychology Branch, Code 455
Dr. M. A. Tolcott

From: Dr. Barbara Gillam Lawergren
for the late Professor Clarence H. Graham
Columbia University, New York, New York 10027

Subject: Final Report of Research and Training under Contract N00014-67-A-0108-C009 between Columbia University and the Office of Naval Research

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1. Contract N00016-67-A-0108-0009 between Columbia University and the Office of Naval Research which was initiated in August 31, 1968, has been terminated. The project was carried out in combination with work on Grant No. 5 RO1-EY-00391 of the National Eye Institute. Funding provided by each organization.

   It is with the deepest regret that we have to report the death of Professor Clarence H. Graham, the Project Director, on July 25, 1971.

2. The mission of the contract was "that research on the factors influencing visual perception, colour vision and movement perception be performed." This mission also necessarily involves the training of student researchers.

   During the three years of the contract nine project reports were submitted to the Chief of Naval Research which detail specific aspects of the research done under the task order. A bibliography of technical reports is appended which comprise an overall account of the project's research findings.

3. There is appended also a list of individuals associated with the research program. Of these Galen Elliott, Ann Colby, and Clarence H. Graham were working the area of colour vision. Unfortunately the sudden and untimely death of Galen Elliott in combination with Dr. Graham's illness prevented completion of this program, and, therefore, there are no recent publications in this area. One earlier publication is listed.

   David Henderson, Stephen Pollard, Barbara Mates and Clarence H. Graham worked on movement perception. Four publications are listed.

   Clarence H. Graham, Barbara Gillam Lawergren, Patricia Daniels, and Jane Braden were concerned with factors influencing visual perception. The concentration has been on investigations of stereoscopic depth perception, the Ames window, perceptual grouping, and the illusions. The research on stereoscopic vision, described in progress reports, is not yet published. Four publications are listed on the other three topics.

A more detailed account of efforts during the past year follows.

4. <u>Visual Perception</u>

1. <u>Ames window</u>. A dissertation, "The effect of varying linear perspective and monocular movement parallax on perceived oscillation of the Ames window," by Jane Braden, based on research carried out under the project, was successfully defended. A summary of the findings follows. Copies of the complete dissertation will be forwarded on request.

A series of experiments were executed in order to investigate the roles of the cues of linear perspective and monocular movement parallax in producing the Ames trapezoidal window illusion. Linear perspective was varied by varying the length of a vertical side of ten rotating stimulus figures. Movement parallax was varied by varying the distance of a light source from the screen onto which the figures were projected. Four projection values were used: parallel projection and three values of polar projection.

The results support Graham's (1936) theory of the Ames window that, when movement parallax cues are ambiguous, the cue of linear perspective is used to resolve ambiguity as to direction of rotation. They also show that:

(a) when the cue of movement parallax is above threshold, the relationship between this cue and the cue of linear perspective is nonlinear;

(b) the variability of subjects' responses increases as the ambiguity of the proximal stimulus increases;

(c) mean number of reversals decreased as speed of rotation is increased, as Borjesson (1971) found;

(d) an increase in the amount of linear perspective will produce a decrease in the angle away from the frontal plane at which reversal is perceived when movement parallax is held constant, although the total angle of perceived oscillation remains unchanged, as Epstein, et al (1968) found.

The results are discussed with relation to the work of previous investigators on thresholds for slant and for movement parallax. The importance of a thorough analysis of the proximal stimulus array for studies on depth and movement perception is emphasized.

2. <u>A new and objective method of defining and measuring "perceptual grouping" of contours.</u>

A paper is enclosed by Barbara Gillam Lawergren entitled "Perceived and common rotary motion of ambiguous stimuli as a criterion of perceptual grouping" which describes the theoretical basis for the method and initial research using it. Further work on the determinants of grouping as defined in this paper shows that

(a) Grouping is greater for two contours viewed monocularly than for two identical contours, each viewed by a different eye.

(b) The more contours included in the stimulus which have a common vanishing point, the stronger the grouping of any two of those contours.

(c) Differences or similarities in the line length of two contours do not significantly affect grouping of these contours.

(d) Absolute as well as relative directions of two contours appear to affect grouping.

(e) An objective measure of contour closure is possible with this technique.

Finding (a) and (c) can be accounted for by the projection area-line detection model developed in the enclosed paper. Findings (b), (d), and (e) must almost certainly be attributed to a higher level of processing. These results are being prepared for publication.

3. Illusions. A paper entitled "A depth processing theory of the Poggendorff illusion" by Barbara Gillam Lawergren is enclosed. Some theoretical developments of this theory for the Ponzo illusion have been tested. It was found that

(a) a Ponzo-type illusion can be induced by a texture gradient

(b) A test line placed on the more compressed part of either the classical or the texture gradient inducing figure is judged larger than an identical line on the less compressed part of the figure for both vertical and horizontal test line orientations. This contradicts reports in literature based on observation. (Humphrey and Morgan, Nature.)

(c) Horizontal test lines show more illusion than vertical testlines for the classical Ponzo inducing figure (two lines converging towards a central vertical axis) whereas vertical test lines are more affected than horizontal test lines for a texture gradient (horizontal lines becoming more compressed towards the top of the figure).

(a) and (c) were predicted on theoretical grounds. These findings will shortly be submitted for publication.

5. Movement Perception

One paper entitled "The relationships among time, distance, and intensity as determinants of motion discrimination" by David Henderson has been published; a copy is enclosed. A second paper by Dr. Henderson, "Visual discrimination of motion: stimulus relationships at threshold and the question of luminance-time reciprocity," is currently in press.

6. Retinal Integration and Differentiation

Dr. Henderson has programmed a computer to control an optical apparatus which presents asynchronous flashes at various combinations of spatial and temporal disparity and to record and analyze subjects" responses. These experiments are aimed at determining the relationship between the retina's integrative capacities and its limits in preserving information about temporal asynchronies in the visual input. Results to date indicate that adjacent flashes of ten milliseconds duration each may be discriminated consistently as successive,and the spatial direction of that succession correctly ascertained, for onset asynchronies of as little as five milliseconds. The results also show as inverse relationship between temporal and spatial disparity at threshold for such a discrimination. The influences of numerous relevant parameters are being investigated.

7. Autobiography of Clarence H. Graham

Clarence H. Graham's autobiography is enclosed. It will appear in Vol. 3 of History of Psychology in Autobiography edited by Gardner Lindsey and describes the development of his ideas and the most significant of his scientific contributions.

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Personnel

Clarence H. Graham deceased July 25, 1971
(Project Director)

Barbara Gillam Lawergren
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David C. Henderson
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Stephen Pollard
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Galen Elliott, Jr.
Research Assistant deceased December 2,1970

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Ann Colby
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Research Assistant

CLARENCE H. GRAHAM

EARLY LIFE AND EDUCATION

I was born in Worcester, Massachusetts on January 6, 1906, the oldest of four children, three boys and a girl. My parents were Irish Protestants from County Donegal. They first met in the United States and were married three years before my birth. My mother had been "sent for" by her aunt, a strong-willed woman who pretty well dominated her small group of relatives in this country until her death at almost the same hour as Franklin D. Roosevelt's in 1945. This grand-aunt of mine had an important influence on my life. She had no children of her own, and I sometimes thought she asserted an unwarranted claim upon my affections. Still, I now recognize that this strong woman did indeed shape me in a way that modulated some influences derived from my quite permissive parents.

When I was five I went to kindergarten in the Cambridge Street School of the Worcester School System. My introduction to school was marred by the fact that, having just suffered a quite severe childhood illness, I was more than usually emotional in my response to my first school experiences. The period of adjustment soon passed, and from then on I became a well habituated pupil in a way that probably had implications for my total life pattern.

My most memorable days in grammar school were in the later grades during World War I. The pupils were, in the days after America's entry into the war, in a chronic state of excitement. The usual topics of the curriculum were taken up, but there was in addition a great deal of singing of patriotic and popular songs. Various circumstances caused uncertainty in the time and place of parts of the school program, and many changes were made in the school sessions, aimed for one thing at saving fuel. What with rationing, Liberty bonds, war gardens, and the countless other things out of the ordinary, the period of World War I was an exhilerating time for the pupils of the Cambridge Street School, before it was finally capped by Armistice Day in 1918. I was awakened by bells and whistles at 2 o'clock on the morning of November 11 with my mother bending over me and announcing that the war was over. I remember that I dressed as fast as I could and hurried to the center of Worcester where the beginning of the celebration was taking place with many bonfires and much milling about. A parade was arranged for the afternoon, in which SATC (Students Army Training Corps) contingents at Worcester Tech, Clark, and Holy Cross Colleges took part, as well as other military groups. Students commandeered the trolley cars, and excitement permeated the crowd as feelings of comraderie and good fellowship prevailed. My memories of VE and VJ days in 1945 are dim by comparison with those of Armistice Day of 1918 when I was a twelve year old boy. I remember thinking that now at last all nations would be saved for democracy, even those wicked ones that didn't want to be.

I was graduated from grammar school in 1919 and went to South High School in Worcester. I think of high school days as a time when learning was easy and my teachers in general were very good. I did not spend much time on homework, and in fact I worked in a grocery store during most of my afternoons. Some nights, I spent several hours in the YMCA gym and swimming pool. I realize now that I may have been busier during this period than I ever was in my life, before or since. My experiences in the public schools of Worcester, grammar and high school, provide a generally happy memory. As I look back I feel that I was lucky to have been taught by such a dedicated group of teachers.

In 1923 I was graduated from high school and spent the vacation months working in the Spring Mill of the American Steel and Wire Company, thereby adding to my financial backlog at the rate of 40 cents an hour for 48 hours a week, quite respectable wages at the time.

My mother's aunt, whom I called Aunt Maggie, was aware of Clark University from nearly the time of its founding and, as a person meagerly educated herself, was respectful of the aura of knowledge and scholarship that existed in its early days under G. Stanley Hall and an initially brilliant faculty. At any rate, she early determined that I should go to Clark. Backed by my own savings and her support, I entered Clark in September of 1923 and spent three years trying to decide what schedule I should follow and what my "major" should be. I changed from chemistry to literature, sampled some other topics, and in my third year took my first psychology course with John Paul Nafe shortly after he had received his degree with Titchener. I can not say that the substance of Nafe's course was memorable, but somehow or other Nafe had a way, not of instructing students, but of treating them as personalities who could be interesting in their own right. That they were worthwhile was further demonstrated to a fortunate few by the fact that Nafe paid considerable attention to them, even becoming their friend. This magic seems to have touched many students, graduate and undergraduate, who later became well-known psychologists.

I finally became a student of psychology and took experimental with Nafe in my senior year. Frank Geldard, two years my senior, was starting work on his doctor's thesis and beginning to recruit subjects and, as it turned out, an experimenter (myself) for his work on light adaptation. I remember that I experienced an almost joyful feeling as I sat in the dim ambient illumination of the dark room, timing the stimulus presentation, hearing the accompanying clicks and later, seeing the data take shape under Geldard's computations. That year's activity surely bent the growing branch in a way that has persisted.

In 1927 I started graduate work at Clark. The staff, in addition to Nafe, consisted of Carl Murchison, Vernon Jones, and most especially, Walter Hunter. Raymond Willoughby, associate editor of Psychological Abstracts, also was an influential force among the students, especially in such areas as developmental and abnormal, which would otherwise have had little representation. Of course,

the two main influences on me were Nafe and Hunter. I soon came under Hunter's influence, especially through his seminars *Animal Behavior* and *Principles of Psychology*. The former course developed in me an appreciation of objective psychology, especially certain criteria of experimentation and careful analysis of problems. Although I responded positively to Hunter's statements, I remember that on one matter I made a choice of systematic approach that neither he nor the other students of the class supported. In our study of the points of view represented by Jennings and Loeb, I favored Loeb. I have since recognized, of course, that in fact Jennings's approach would probably be favored by many or most students of behavior, but even then I had the feeling that for me an account of behavior would require some more explicitly analytic and testable variables as objectives of study. And so, it is probably not surprising that my interests in sensation and perception have been along the paths laid out by such workers as Helmholtz, Maxwell, Hering, Mach, and, to repeat, Loeb.

In my last year of graduate work I took Hunter's *Principles of Psychology*, a course that reflected Hunter's early training in philosophy. By this time, 1929, Hunter had become stabilized in his systematic orientation; he was a behaviorist and I believe he had thought his position through, perhaps not to the degree of subtlety exhibited more recently by J. R. Kantor, but he surely provided heady fare for students. He never aimed at indoctrination for the sake of personal support; that is not to say that he would not argue for his point of view. With him, one had to know not only wherein one disagreed, but also why. I particularly remember a discussion of a paper of his own that he had assigned, "The subject's report," (Hunter, 1925). I was of the opinion that the subject's report is a response to be treated from the point of view of psychology like any other response. Hunter disagreed, probably, as I recall, on the basis that the manner of specifying criteria in the two cases would be greatly different.

I did my thesis with Nafe, but unfortunately he was absent on sabbatical leave in Europe during my final semester. Nevertheless, I finished the thesis, a study of binocular summation at threshold (Graham, 1930). I found little or none. Years later, Leonard Matin, my colleague and former student at Columbia, studied the same problem by an improved technique and was able to show the existence of a very small effect over a small portion of the relevant psychophysical function. (Matin, 1962).

During my graduate student days at Clark I found myself in good company. Other students who were there for various periods at that time were Frank Geldard, Roberta Pardon, Dorothea Johannsen, Harry Israel, Wayne Dennis, Norman Munn, Louis Gellermann, Mason Crook, and Robert Leeper, the last five of whom received their degrees when I did in 1930.

TEMPLE, PENNSYLVANIA AND THE JOHNSON FOUNDATION

Nineteen-thirty was a hard year for academic jobs (it was the first year of the Depression). I applied without success for a National Research Fellowship early in the year (Louis Gellermann of my group did receive one), and Robert Leeper, Mason Crook, and I

tried by various devices to get in touch with persons who might have or at least know about an opening for a young psychologist. Finally, Crook received an appointment at Dartmouth and Leeper, at Arkansas. The summer dragged on, and near its end Leeper received a letter (in reply to about one hundred he had written) telling him of an opening at Temple. Would he be interested in applying? Since he had already agreed to go to Arkansas, he wrote that he had a colleague by the name of Graham whom he would like to nominate. Shortly thereafter, I met Thaddeus L. Bolton, chairman of the department at Temple, and in September, 1930 I was appointed to a one-year term at Temple to take the place of a staff member who was completing his Ph.D. work at Ohio State.

I liked Bolton very much, a feeling that was enhanced by the fact that I knew something about some of his early papers and also that he had been at Clark in the early days. He was one of the very early behaviorists before Watson, and dealth, I think, in an insightful way with an objective version of perception. He was an outstanding scholar, but his interests turned in the 20's to administration during tenures at Nebraska and Temple. For a short while he was president of the latter.

Of the staff members at Temple, I had more in common with Hughbert Hamilton than the others;he was an experimentalist from Columbia and, until recently, has guided the experimental program at Temple. He has been succeeded by Philip Bersh, with whom I have had many stimulating discussions when he was a graduate student at Columbia.

At the beginning of the academic year, I went with James Leach, a Temple biologist, to visit some biology laboratories in which Leach was working for his degree at the University of Pennsylvania. During that visit, I went to the physiology department where I met H. C. Bazett. Bazett and I had a long talk during which he told me about some of his recent work on touch spots and then about some related work by Dallenbach. He was extremely cordial and seemed happy to discuss sensory research with a kindred soul. In the course of the discussion, he suggested that I meet a young Finnish-Swede from Helsingfors named Ragnar Granit who jad just arrived the year before to work in the Johnson Foundation for Medical Physics. (I later learned that Granit had worked in psychology with Gelb before he received his medical degree.) The Johnson Foundation was then headed and directed by Detlev W. Bronk, who only recently has retired as President of the Rockefeller University and a few years earlier as President of the National Academy of Sciences.

At any rate, I soon met Granit and Bronk, and both seemed happy to have me work in the Johnson Foundation when my fifteen-hour-a-week teaching schedule at Temple would allow. I'm afraid that I often spent more time at the Johnson Foundation than I did at Temple. This was possible since my teaching took place between 9 a.m.and noon five days a week. My teaching of four courses in General Psychology and one in Educational Psychology may have suffered during this regime, but Bolton did not seem to think it was totally lamentable. At mid-year he told me that he would support me for an additional

year despite the fact that the man I had replaced would be returning. By this time, however, I had made up my mind to apply again for a National Research Fellowship so that I could spend full time on research at the Johnson Foundation. I was supported in my application by Granit, Bronk, and Bazett. I heard that I received the fellowship in February, 1931 and thereupon told Belton that I would not be at Temple after June.

At just about that time John Paul Nafe was offered the chairmanship of the psychology department at Washington University, St. Louis. The department at Clark tried at first to replace him with some well established psychologists but soon gave up the search in the face of a shortage of funds that developed as the Depression progressed; they instead decided to find a junior staff member and offered me the position. I was, of course, unable to accept the Clark appointment for 1931-32, having already accepted the NRC Fellowship as of July 1, 1931. The problem was resolved by making my appointment start as of July, 1932. Very importantly, it turned out that some of the funds saved by this arrangement could be used to refurbish and supply the Clark laboratory during 1931-32 in preparation for my arrival.

During all the academic year 1930-31, I taught at Temple and worked with Granit at the Johnson Foundation. The main result of our research was a joint paper involving a determination of critical flicker frequency under various conditions (Graham & Granit, 1931). We found that each of two adjacent semicircles equal in area and luminance gives, in the presence of the other, a critical frequency that is higher than either one alone. The effect was interpreted to represent a process of retinal summation. On the other hand, when the luminance of either of the two is reduced (while the other remains the same), the critical frequency of the dimmer semicircle is essentially unchanged from its value alone. The absence of change in critical frequency under conditions of different luminances was interpreted to mean removal of summation by inhibition of the dim semicircle by the brighter. This type of finding was later expanded by Granit and myself (individually and with students) in experiments on inhibition.

Late in my second semester at the Johnson Foundation, Keffer Hartline returned from a three-year study of physics at Baltimore and Munich following his medical degree at Hopkins. Thus were Granit and Hartline, two future Nobel Laureates, brought together in the same laboratory. Hartline had done a number of studies of electroretinograms in the vertebrate eye, and (a foretaste of his future research) the eye of *Limulus*, the horseshoe crab which existed in profusion at Woods Hole. I spent the summer of 1931 there with Hartline, during which time he, with some help from me, succeeded in recording impulses from single fibers of the *Limulus* optic nerve, thereby establishing it as the almost perfect preparation on which to test simplified models of photoreceptor activity (Hartline & Graham, 1932).

In September of 1931 we went back to Philadelphia, and I spent the time until June largely in learning some experimental techniques and principles of equipment design. I also did more work on flicker.

along the lines of the earlier work with Granit. This work was not published.

During this time things were progressing with the refurbishing of the laboratory at Clark. Carl Murchison, the chairman, took a personal interest in my plans, and spent a great deal of effort supervising the construction of two large dark rooms, a photographic dark room, the installation of a direct current generator, and the purchase of optical and electrical recording equipment that I had ordered.

CLARK

I returned to Clark in July of 1932 and spent a leisurely summer preparing the laboratory and developing some new equipment for work on flicker and threshold stimulation. My work began in earnest in September with the teaching of a number of courses, including General Psychology, Experimental Psychology, Sensory Psychology, Selected Advanced Topics, and a new course (based on work I had had with Professor H. M. Jacobs, the general physiologist at Pennsylvania), Quantitative Treatment. This course was concerned with curve fittings, rates, and integrals in problems with physiological and psychological applications.

I soon became engrossed in the work of my students, who in those days were expected to start work on their M.A. theses well before the Christmas after their arrival. I began working with Robert Beitel, J. Roy Smith (both of whom later became physicians), Robert J. Brown, Harry Karn, and Elaine Foraker. In the approximately nine months of their first year these people had to carry out a short experiment, learn how to develop equipment, run subjects, and write an acceptable essay. The benefits of this procedure would become clear in their third year when they worked on their Ph.D. theses, and even in their second year in time left over from Hudson comprehensive course in physiology which all of them took.

In September, 1932 Carl Murchison asked me to write a chapter for his Handbook of General Experimental Psychology in the hope that I might produce a manuscript that would take the place of the one by L. T. Troland, who had recently died. As it turned out, Troland's chapter was completed before his death, and I wrote one that did not repeat any of his material. My chapter, entitled, "Vision: Some neural correlations," (1934) gave an early statement of a topic, interaction, which has assumed new vitality in the last twenty years, and which has been shown to play a particularly important role in almost all types of visual processes including adaptation. This chapter and my interest in this topic probably influenced my students as three of them at this time did theses on interaction.

Lorrin Riggs entered Clark as a student in 1933, and soon showed interests in line with my own. We put together an amplifier and string galvanometer (the original used by Lillie for his iron wire model experiments and lent to us by Hudson Hoagland) with which we determined the white rat's rod luminosity curve (Graham & Riggs, 1935), and later, with Edward Kemp, a postdoctoral student, the rod curve for the pigeon (Graham, Kemp, & Riggs, 1935). Later, Riggs used the amplifier-string galvanometer equipment to study the effect of preadaptation in the frog electroretinogram (1937). This work

showed that Kohlrausch's electroretinograms of various shapes, which he attributed to color vision processes, could be reproduced by changes in intensity. In the same year, Keffer Hartline and I completed research on the luminosity curve of Limulus (Graham & Hartline, 1935). I commuted at irregular intervals between Worcester and Philadelphia in order to do this.

BROWN

In 1936 Walter Hunter was invited to Brown University to become chairman of psychology after Leonard Carmichael moved to Rochester. Edward Kemp, who had spent the year 1935-36 on an NRC Fellowship with Hallowell Davis, had already been hired by Carmichael and was at Brown in September when Hunter arrived, accompanied by J. McV. Hunt, who had just completed an NRC Fellowship at Worcester State Hospital and thus was well known to us at Clark. With Hunter from Clark went Raymond Willoughby, in his role as associate editor of the Abstracts, and myself, newly appointed Assistant Professor at Brown. Harold Schlosberg and Herbert Jasper welcomed us on our arrival in Providence. After the inevitable short period of adjustment, we found ourselves at home in Providence. As compared to Clark, the greatest difference lay in the size of the undergraduate program, which at Clark had been quite small. The program at Brown had been developed by Carmichael and was quite popular. As I remember it, the first year course contained about 630 students, approximately 130 Pembroke women and 500 Brown men. I taught sections of about 130, some semesters men, others women. A good deal of care was given to this course, and I found that I had to pay attention to my teaching in a way that I never had to before. The other courses I gave at Brown were seminars, mainly on topics similar to those I had given at Clark, with the addition of a course of a systematic and theoretical sort.

Once the instructional program was under way, we were ready to organize our research. Kemp and I (Graham & Kemp, 1938) began some work along lines developed at Clark in an earlier study of acuity. The investigation was concerned with the influence of (tau) duration ( ) of the intensity increment (I) in brightness discrimination. The durations used extended from to 2 to 500 msec. The general findings may be symbolized by the relation I · = cf (I) for a value of smaller than a critical duration c and by the relation I. c = f (I) for values of equal to or greater than c. In these equations I represents prevailing intensity and the term f (I) may be taken as equal to an expression in Hecht's brightness discrimination equation.

This study was important because it proposed a theoretical formulation of two effects, brightness discrimination and its dependence on the duration of flash increment as described by the Bunsen-Roscoe law of photochemistry.

During these early years at Brown, I had the pleasure of working with or knowing many students who later became well known psychologists: Robert Gagne, Neil Bartlett, Fred Mote, William Verplanck, Margaret Keller, Charles Cofer, Frank Finger, and Parker Johnson. With Gagne, I experimented on latencies in the running behavior of the white rat (Graham & Gagne, 1940), a project on which I had made preliminary observations before leaving Clark.

With Fred Mote and Bob Brown (who came for a summer from Clark) I worked on the area-intensity problem in vision (Graham, Brown, & Mote, 1939). Our formulation attempted to show why the intensity threshold in both the fovea and periphery decreases as stimulus area increases according to a rule involving the integration of elemental excitatory processes. The experiment accounts for the manner in which photochemical and interaction processes combine to predict the observed trade-off relation existing between area and intensity at threshold.

The years at Brown from 1936 to 1940 were some of the happiest of my life. The members of the staff included Donald Lindsley who came in 1938 to succeed Herbert Jasper after the latter went to Montreal, Carl Pfaffman who came from the Johnson Foundation in 1940, and Lorrin Riggs who took a year's leave of absence from the University of Vermont in 1938. During that year Riggs and I worked on Limulus. In 1941 he came back to Brown for further experiments on Limulus and stayed there on NDRC work until his appointment in 1945 to succeed me. The Brown psychologists were an unusually compatible group. Each member showed interest not only in his own area, but also in other fields, thereby producing a broad spectrum of scientific tastes.

The 1930's passed into the 40's and brought with them World War II, first signalled by Munich, and then the Fall of France and the Battle of Britain By this time America started to bestir herself and among the first activities that took place was the emergence of the National Defense Research Committee (NDRC) organized as part of the Office of Scientific Research and Development (OSRD) under Vannevar Bush to mobilize the scientific talent of the United States for work on projects related to defense. Psychology was late in being represented. In 1940 the Committee on Service Personnel-Selection and Training was formed under the Psychology Section of the National Research Council, of which Leonard Carmichael was then chairman. John Stalmaker became the Committee chairman, and projects were developed in branches of the armed services. The Committee soon gave way to the Applied Psychology Panel of NDRC. The organization and work of the early Committee has been summarized by Bray (1948) in his account of the work of the Applied Psychology Panel as the latter developed with enlarged membership under Walter Hunter as Chief. The members of the Panel served as supervisors, administrators, and research personnel. My role on the Panel was mainly in programs relating to the control of gunfire, in which I was ably aided by several other psychologists, in particular, W. T. Kappauf, W. G, Brogden, William Biel, and Douglas Ellson. In addition to the work on gunfire control, I supervised the development of a Personal Inventory, set up with Walter Shipley, as a device for selecting emotionally unstable personnel in various types of military activities. The inventory was used to expedite the work of psychiatrists in selecting recruits at Newport Naval Training Station and elsewhere as well as submarines at New London.

The work on the Personal Inventory may owe something to my experience in psychoanalysis for a number of years following 1936. I shall not attempt to evaluate in detail the effect of psycho-

analysis on me. I suppose that a major effect was that I became less obsessed with it as I progressed in the program. In any case, it is of interest that I, a psychophysiologist, should feel that the problem of emotional stability should be at least minimally examined at a time when psychologists in the area of selection and psychometrics felt that this approach promised little. In fact, it turned out better in the military situation than had been expected, probably because men could express their affects more freely than usual and might gain secondary benefits by so doing.

My first experience with work under NDRC sponsorship had begun earlier in the spring of 1941 when I began research under Division 7, the fire control division, on problmes of height finder and binocular range finder operations. I continued work on problems of the binocular range finder until 1945, although after 1942 this instrument assumed a largely standby role for radar. Nevertheless, Division 7 felt that work on it should continue.

My experience with war research acquainted me with several new areas of activity. Mainly, I learned about a larger than usual research organization and the problems arising therein. For a period at Brown I administered a program that had about 150 psychologists and technicians in projects at about twelve widely separated establishments.

A number of graduate students at Brown participated in NDRC work. Among them were Richard Berry, Richard Blackwell, Conrad Mueller, Richard Solomon, Eliot Stellar, and some of those mentioned earlier who had by this time received their degrees: Bartlett, Mote, and Verplanck. Most of these participated for one, two, or three years at most. They were eventually taken into the Armed Forces or moved to other projects as needs for experimental personnel developed. Finally, in 1945 V-E day arrived and then in August came V-J day following the explosion of the atomic bomb.

In January of 1945 I had been approached by Henry Garrett of Columbia who sounded me out on the possibility of my going to New York in September. I discussed the prospect with Walter Hunter, and he and I finally agreed that I should probably move. I very much regretted leaving Brown, but it seemed that a move would be for me, as for many other psychologists, an inevitable consequence of the demand for new personnel after the war. Hunter knew what was going to happen. Within a year following the end of hostilities, he saw the department which he had built after 1936 dispersed. Donald Lindsley, J. McV. Hunt, and I moved to other posts, as did Stanley Williams who had been at Brown for a short time early in the war. I was happy to know that Lorrin Riggs would continue at Brown after his NDRC work. Today only he remains from the group that was at Brown in the 30's and 40's. Hunter liked to say that he never regretted seeing a good man leave him to take on a job with greater responsibility. He felt that both psychology and the man would benefit from the change; and as for himself, while he might have personal regrets, his major concern was the welfare of science. Several of us who had left Brown at that time were reunited at the dedication of the Walter S. Hunter Laboratory of Psychology in 1958 when honorary degrees were awarded to Hunt, Lindsley, and myself

COLUMBIA

When I arrived at Columbia, Woodworth had just retired, and I began teaching the Advanced Experimental course that he had been associated with for many years. My emphases were largely on the topics of sensation and perception together with excursions into such systematic, theoretical, and historical treatments as centered around them. In a word, the topics that interested me as a graduate student still interested me. I also dealt for a short time with some aspects of learning, but because this topic was covered extensively by Fred Keller and Nat Schoenfeld, I felt there was little need for it in my course.

In September, 1945 the members of the staff consisted of Garrett as chairman, Poffenberger, Landis, Warden, Keller, Klineberg (who was on leave of absence in Brazil), and Schoenfeld. Others were on appointment in General Studies, including Woodworth who continued to lecture to large classes in his two famous courses, Contemporary Schools of Psychology and Dynamic Psychology.

There were very few graduate students at Columbia in 1945, but the post-war influx started with a vengeance in 1946. Thereafter, for about five years I had what I thought was a large number of students, as many as eight or ten working on doctoral theses at any given time. Other members of the staff also had more than the usual number. The number of students per staff member has decreased since those days because of fewer entering students and also larger staffs.

My own and my students' research was well supported. At the time of Selig Hecht's death in 1947 (the end of the year in which he and I enjoyed giving a seminar together), the University allowed me to transfer the equipment of Hecht's laboratory to psychology. And so we came into possession of some historically important as well as useful equipment (the Hecht-Shlaer-Pirenne monochromator used in the research on quantum requirements at threshold, the Shlaer acuity apparatus, the Hecht-Shlaer adaptometer, and other valuable equipment). The Office of Naval Research has supported my activities from 1947 to the present. I shall not say that our funds were ever large. For example, until 1968 ONR funds never exceeded $26,000 a year, and this figure included part of Yun Hsia's salary, an amount that was supplemented by a lectureship in General Studies. Hsia, a Woodworth Ph.D. who had done postdoctoral work with Hecht, was my colleague for 21 years, from 1947 until his death in 1968. When it came to color vision, his contribution to students and collaborators was great. He has been greatly missed at Columbia.

Starting about 1948, Hsia and I performed a number of experiments on normal and color blind individuals. These studies we believed established some new facts, particularly as related to luminosity (i.e., spectral brightness) functions. The first of the series involving determinations of quantal energies was performed on several normal subjects (Hsia & Graham, 1952). The results showed that luminosity is at a maximum near 560 nm with the data showing decreasing values into the extremes of the spectrum along curves that showed variations in structure. In particular, they showed a well marked depression near 450 nm and a smaller one near 500 nm. These depressions were taken to represent processes attributable to color

fundamentals.

Another series of experiments (Hsia & Graham, 1958) was performed on color blind subjects under conditions identical with those used on the normal subjects. The color blind group consisted of two classes of dichromats, protanopes and deuteranopes, individuals who can match any spectral color by a mixture of two primaries. As compared with the normal subjects, the protanopes showed a loss of luminosity in the red. In contrast with some previous interpretations but in conformity with earlier estimates by Hecht and Hsia, five of six deuteranopes showed a loss of luminosity in the green. The implications of this finding are important and will be discussed in connection with our work on a unilateral dichromat.

A young woman at Barnard College (Graham, Sperling, Hsia, & Coulson, 1961) was found to have trichromatic, probably normal, vision in one eye, and dichromatic vision in the other. Such a subject, referred to as unilaterally dichromatic, is very rare. Although about ten have been studied with some profit in the last hundred years, the technical methods and equipment used have not made possible dependable classifications in most cases. In addition, the fact that the subjects could not be observed over sufficient periods of time precluded extensive analyses. Analysis of unilateral cases of color blindness is important to color theory, because it is only in the case of the unilaterally color blind person that one can infer what colors a color blind person may see in terms of normal vision.

Hsia and I (Graham & Hsia, 1958) examined absolute luminosity for this subject who had trichromatic vision in her right eye and dichromatic vision in her left, and found that her right eye was considerably more sensitive in the blue and green than the color blind eye; both eyes seemed to be equally sensitive to the red.

Another matter of theoretical importance concerned the colors seen by this subject in her dichromatic as contrasted with her trichromatic eye. An experiment on binocular color-matching was performed and the results indicated that the subject saw only two hues in her dichromatic eye. She matched all wavelengths greater than her neutral point (the wavelength seen as white at about 502 nm) by a yellow of about 570 nm seen in the trichromatic eye. She matched wavelengths shorter than the neutral point by a blue of about 470 nm seen in the trichromatic eye.

The facts so far described raise an important theoretical problem for dichromatic theory: how can that eye see yellow if sensitivity to green is lost, either totally or to a major extent? In addition, one might ask about the mechanism by which a single hue of blue is seen for all wavelengths shorter than the neutral point. Nothing will be said about the latter question, but the problem of yellow must be considered.

Let it be supposed that in deuteranopia, for example, the subject's usual red receptors become attached to cells that signal central yellow, while the green receptors signal a similar yellow. The net result of this arrangement is that either red or green wavelength stimulation will be seen as yellow. The precise modifications in the absorption spectra of receptors that are required for appropriate losses are readily specifiable. Further work on

spectral brightness matching and flicker was performed by Wm Berger in collaboration with Hsia and me (Berger, Graham, & Hsia, 1958).

About ten years after the work on dichromats and the unilaterally dichromatic subject, Hsia and I participated in an experiment with Harris Ripps and Irwin Siegel (Siegel, Graham, Ripps, & Hsia, 1966), two former Columbia students who are now research scientists in the Laboratory of Ophthalmology at New York University Medical Center. The young woman on whom we made determinations had been previously studied by William Rushton of Cambridge University, while he was in the United States at NIH. Rushton demonstrated that the subject was totally color blind at low photopic luminances; thus, it was presumed that she exhibited exclusively rod-type vision. In fact, Rushton demonstrated a single type of dark adaptation curve representing rod function. Our examination of the subject showed that although her vision was totally color blind at low photopic luminances, her results on the Farnsworth test showed that she had a tritan defect ("blue blindness") which was superimposed on a generalized reduction of cone sensitivity. This investigation was important in showing how an almost totally color blind subject may show some degree of dichromatic effect (in this case, a tritan defect) against a background of reduced cone sensitivity.

The last study on which Hsia and I collaborated (Graham & Hsia, 1969) was one on the foveal achromatic interval, the difference between the logarithms of the foveal chromatic and achromatic thresholds. We found that the curve for the chromatic threshold has the form of a curve of colorimetric purity as a function of wavelength; that is, it is the reciprocal of a saturation curve; it does not have the form of a luminosity function. The curve for the achromatic threshold is below and shaped differently from the curve for the chromatic threshold.

It is pointed out that colorimetric-purity thresholds and chromatic thresholds seem both to be reciprocal measures of saturation. Chromatic thresholds may have an advantage in estimating saturation because they do not involve a physical mixture of color with white. This is not to say that an achromatic component (white) is not involved; the chromatic and achromatic components are intrinsic.

Since doctoral theses of graduate students often mirror the interests of the sponsor, it is probably not surprising that several of my students have performed experiments in color. Among these are Shakantala Balaraman, who collaborated with Hsia and me (Balaraman, Graham, & Hsia, [illegible]) on some experiments and wrote an historical account of color blindness (1962), Aleeza Beare (1963) who performed some important experiments on color naming, and Joan Pollock (1966) who determined human luminosity curves by means of a reaction-time study that was co-sponsored by W. J. McGill. Gerald Howett and Gary Yonemura have continued their interest in vision by working for many years in color at the National Bureau of Standards.

These experiments on color by students may be taken to illustrate a principle that I have adhered to in guiding students during the initial stages of a doctoral project. So far as I could direct the choice of thesis subject, I tried to come to some agreement with the student on a topic that might be in fact an in[illegible]

duction to a program of research in which one segment would lead to another. I have found that most students soon get interested in their topic and very often follow it intensively with useful, sometimes important, results.

I have sometimes thought that I have not followed my precepts in my own research. It is true that I have concerned myself with vision and visual perception, but it is clear that within my chosen field I have taken part in a considerable variety of experiments ranging from electrical recording to work on perception of the Ames window. The work with Yun Hsia on color vision took place at Columbia from 1948 to 1968, and a different set of studies took place following the paths laid down in my work with Granit and in the work by myself and students at Clark and Brown. The latter experiments involved primarily such topics as interaction and various psychophysical parameters of visual discrimination. I have in mind such experiments as the one at Clark with Carolyn Cook (Graham & Cook, 1937) on intensity-time parameters of acuity and the study with Kemp at Brown on intensity-time relations in brightness discrimination (Graham & Kemp, 1938). An experiment of the same sort is the one by Graham, Brown, and Mote (1939) on the area-intensity relation, also done at Brown. Such experiments are important because they deal with essential variables that theory should account for. At Columbia, I participated in two experiments that had some of the characteristics of the studies at Clark and Brown, the first a study in dark adaptation with John L. Brown, Howard Ranken, and Herschel Leibowitz (Brown, Graham, Leibowitz, & Ranken, 1953).

Luminance thresholds for the visual resolution of various widths of alternating light and dark lines were determined at various times in the dark. The finest gratings, representing high degrees of visual acuity, showed only a single cone curve that dropped quickly from a high luminance threshold during the first moments of dark adaptation to a final steady level. Coarse gratings produced a duplex curve that showed an initial cone portion and a delayed rod portion. The higher the criterion for degree of resolution, the higher the dark adaptation threshold.

An interesting experiment on parameters of brightness discrimination with particular reference to interaction was performed by Philburn Ratoosh and me (Ratoosh & Graham, 1951). The experiment antedates studies on the excitatory and inhibitory interactions of adaptation stimuli, a problem that has received considerable attention in recent electrical recording work on lower animals as well as in recent psychophysical experiments

We studied the effect on foveal brightness discrimination of changes in the diameters of the test- (ΔI) and adapting- (I) fields. With respect to the effects of area, two findings emerged from the results: (a) for a given brightness and constant test-field size, brightness discrimination improves as the size of the adapting-area increases; and (b) for a given brightness and constant adapting-field area, brightness discrimination improves as the size of the test-field increases until an optimal size of test-field is attained; beyond this size, an increase in the size of the test-field may result in higher values of Weber's fraction, $\Delta I/I$. An interaction hypothesis was advanced that seemed to account for the effects of area and luminance in this experiment.

MORE ABOUT COLUMBIA

My graduate students performed a considerable number of experiments on psychophysical parameters. Among the group were Leonard Diamond (1953) who worked on contrast, Celeste McColloch (1955) on Mach bands, Howard Baker (1949) on light adaptation, Robert Herrick (1956) on intensity discrimination, John Coulson on Fechner's Paradox (unpublished), Joel Pokorny (1968) on acuity, Munehira Akita (1964) with Hsia and me on color contrast, George Kerr on acuity and several others. Conrad Mueller (1951) independently examined some implications of Poisson distributions of increments in luminances for brightness discrimination.

Other experiments by students produced useful data on space and movement. I have in mind, for example, an experiment by Florence Veniar (1948) on the just discriminable distortion of rectangular figures, which gave the unusual result that, for this sort of shape discrimination, Weber's law holds over the total large range of prevailing stimulus dimensions investigated.

Another area of interest was investigated by Elaine Hammer (1949) to whom I have been happily married since 1949. Her thesis on temporal influences on figural after-effects studied their development as a function of exposure duration of an inspection figure and their decay as a function of the time interval between the presentation of inspection and test figures.

I first became interested in movement discrimination after World War II, when I felt that data on thresholds for monocular movement parallax were badly needed. Monocular movement parallax is a strong and basic cue for depth. When a human being who is moving through space fixates an object that is not moving, a changing difference angle exists between the lines of sight to the fixated object and another object. If objects in the environment move while the observer's eye remains motionless, a comparable condition of changing difference angle exists to the principal line of sight.

We determined the threshold monocular movement parallax (i.e., differential angular velocity) as a function of the prevailing rate of stimulus movement in the subject's frontal plane (Graham, Baker, Hecht, & Lloyd, 1948). The subject was instructed, except under certain conditions, to follow with one eye the movements of stimuli, e.g., two vertical needles, and to fixate the small region of separation between them. With his eye following the movement of the needles, the adjusted the lower needle until the two needles appeared to be in the same frontal plane.

The threshold differential angular velocity varied with background luminance, with the rate of the prevailing standard needle movement, and with the visual axis of movement. It is surely true that small differences in angular velocity may be discriminated; thresholds are of the order of 30 to 60 sec of arc/sec. It is of great interest that discrimination of space by monocular movement parallax can be made in any axis of vision, whereas stereoscopic vision occurs only for conditions of disparity in the horizontal axis. Similar results were obtained by a method directed toward another objective by Aubert (1886) and by my former student Father Richard Zegers (1948).

The circumstances in nature to which differential movement discrimination apply are, of course, manifold. A differential angular velocity exists, for example, between any pair of arbitrarily selected points on a moving object every time that object changes its orientation in space. A striking manifestation of the effect is exemplified in Ames' rotating trapezoid window. The trapezoid window consists of a flat surface cut out of wood or metal, painted to resemble a window with its panes of glass and other characteristic features. One side of the window is larger in the vertical dimension than the other. The window rotates at a constant rate about a point that is nearer the short end than the long end. As the subject looks at the window he observes that instead of seeming to rotate in a circle about the center of rotation, the window appears to oscillate back and forth, centering about the perpendicular to the frontal plane.

The stimulus conditions providing this type of apparent movement have been discussed by me (1963) in terms of an analysis based on two types of cues: (1) the differential angular velocities subtended at the eye of the subject by selected points on the surface of the window, and (2) linear perspective provided by the long and short vertical sides. It has been shown (theta) that the angular differential velocity [illegible] at the eye is, for counterclockwise rotation, negative for certain positions of the points behind the frontal plane and positive for appropriately disposed points in front of the frontal plane. In consequence of these conditions, the subject cannot tell whether the points on the window are approaching him in front of the frontal plane or moving away from him behind it. Under these circumstances, how does the subject see the movement of the window? The answer is that he resolves the ambiguity of movement parallax cues by depending on perspective cues. The short side seems to move alternately toward and away from the subject behind the frontal plane with a periodic oscillating motion over an angular range averaging 130° behind the frontal plane. The long end seems to move in a comparable manner in front of the frontal plane. Thus, the oscillation seems to be a movement through nearly half a circle and a return at a rate which is the rate of physical rotation.

In a recent study, my colleague Barbara Gillam and I (Graham & Gillam, 1970), tested the theory that reversals in the Ames window are the outcome of a resolution of ambiguous differential angular velocity cues by linear perspective cues. A parallel projection of the window on an opal glass screen was used as the stimulus. Our subjects almost always reported two apparent reversals per rotation. The short vertical side of the figure was always apparently in front of the long vertical side. These results were interpreted to be in line with theoretical expectations.

Other work on various aspects of space and movement has been carried out by my former students: Alfred Lit (1949) who studied the Pulfrich effect extensively starting with his doctor's thesis, Vivianne Smith (1959) who studied fusion perception for parts of a small area, John Foley (1961) who worked on the Luneberg theory of space discrimination, and David Henderson (1970) who performed a dimensional analysis of the threshold stimulus for motion discrimination.

Visual movement is a discrimination which, if one is to judge by the results of electrical recording studies, exhibits elaborate and complex types of neural interaction phenomena, especially of an inhibitory nature. This type of mechanism seems to provide the basis for the directional sensitivity shown by certain cells of the retina (Barlow & Hill, 1963).

Barbara Mates, a former studnet, and I have reported (Mates & Graham, 1970) new data on the velocity threshold for real movement. As the length of a narrow stimulus object increased, width remaining constant, we found that the velocity threshold also increased. These findings were discussed in relation to some psychophysical experiments by Diamond on brightness contrast, where it was shown that the dimming by contrast (with an inducing field of higher luminance) of a test field was a function of length of an inducing field of constant width.

The effect of stimulus length on threshold is analogous to the findings of Barlow and Lovick (1965) on the effect of size on motion. They say, "If the image of a moving object spreads outside the receptive field onto its surround there are fewer impulses than when it is confined to the receptive field alone. This must be the inhibitory mechanism that elevates the threshold for large compared with small spots and it is presumably different from the inhibition responsible for sequence discrimination." From this point of view, then, the longer stimulus line should provide a higher threshold than does the short stimulus line. One possibility concerning this relationship was suggested in my Tillyer Medal address at the Optical Society in 1963: "Increasing the size of a bright object on a darker background at prevailing low rates increases...the rate threshold. This may mean that an increase in length of the moving stimulus provides, over its length and in adjacent unstimulated areas (particularly at the leading and trailing edges), a change (in interaction effects). The effect a given (contour) point is, presumably, increased (or decreased, depending on contrast relations) by virtue of the greater number of converging units that exist with longer lengths of stimuli than with shorter."

THEORETICAL FORMULATIONS

From my days as a graduate student, theoretical and systematic problems have played an important role in my thoughts about psychology. The article, "Psychophysics and behavior," (1934) published while I was at Clark, (but first formulated in 1932 while I was at the Johnson Foundation) served as the introduction to a number of articles concerning the field of perception and its specification as an area of objective psychology. At Columbia I published the second article of this sort, entitled, "Behavior, perception, and the psychophysical methods" (1950). After the latter came out, I gave an invited paper at an APA symposium in which I attempted to interpret the systematic status of some recent experiment in terms of formulations given in the two papers (1952). I chose to discuss Kohler's experiments on figural after-effects, Bruner's experiments on estimates of the value of coins, and Luneberg's theory of non-Euclidean space. In my treatment I revised some terms that often implied aphenomenological context and

attempted to recast them in an objective formulation. It became clear to me after reviewing the papers mentioned that a more elaborate account of these (and the systematic discussion that appeared in short form in my chapter in Stevens' Handbook, 1951) would be required to do even approximate justice to a formulation of perception in an objective psychology. I took advantage of my presidential address at the Eastern Psychological Association meeting in 1956 to discuss the topic, "Sensation and perception in an objective psychology" (1958). In this paper I tried to give a broadened account of the psychophysical experiment, discussing it in wider terms than the usual methods of constant stimuli, limits, and adjustment. I considered, for example, an experiment on color naming classifiable as a psychophysical investigation. I also tried to give a treatment of introspection as it contrasts with the usual psychophysical experiment. Other aspects of the discussion concerned specifiable differences that underlie sensation as contrasted with perception. Surely, I decided, no operational difference exists between the natures of the two. As for differences which concern matters of definition and specification, that may be another story. It would seem that elements may be included in either class on the basis of whatever criteria are justifiable; however, some empirical basis must underlie the cataloging.

It is probable that little more need now be said concerning my thoughts about perception in an objective psychology. A number of additional treatments have been offered on other aspects of this topic, but they will not be summarized here. It is sufficient to say, I think, that the field of perception may become co-extensive with psychology, and the field of objective psychology can, at least symbolically, become co-extensive with perception, in terms of the stimulus-response formulation $R = f(a, b, \ldots \underline{n}, \ldots \underline{t}, \ldots \underline{x}, \underline{y}, \underline{z})$, where R is response, the first letters of the alphabet refer to stimulus variables, the last letters to conditions of the organism, and n and t refer to number and time, respectively.

JAPAN

My two most noteworthy travel experiences in connection with psychology began in the same year, 1952. In the summer of that year, I was invited by the University of Illinois to participate in its Program in American Studies at the University of Kyoto, sponsored by the Rockefeller Foundation. Five of us left San Francisco for Kyoto in early August and returned seven weeks later having participated in a program of studies designed to present examples of American programs in various fields: Political Science; American Literature; Education; Economics; and Experimental Psychology. I was very fortunate to conduct a seminar with a group of young psychologists who varied in academic rank from instructor to associate professor and were assembled from the major universities of Japan. Senior professors also attended frequently. We took up mainly topics of vision, visual perception, and conditioning. I soon found out that my main contribution was in discussions of sensation. In other areas, particularly perception, the Japanese participants, with their extensive acquaintance with the German

literature, exhibited a very clear and knowledgeable background. They were well versed in conditioning and our sessions consisted of lively exchanges of views. The seminars provided a warm and pleasant interlude on both sides and the feeling of mutual good fellowship has lasted to the present time. I have greeted a number of Japanese professors at Columbia for stays of two days up to a year. I particularly remember the visit of Professor Motokawa of Tohoku University for a month. I have a warm place in my heart for some younger men: Professors Indow of Keio, Oyama of Chiba, and Akita of Kyoto Technical University. I was glad to introduce Professor Indow to Professor Stevens at Harvard and they had a profitable and pleasant three years together. Professor Oyama's work is known in the American literature, and he spent a year at Columbia, where the elegance of his experiments impressed us all. Professor Akita spent four years as a student and post-graduate fellow at Columbia (he received his Ph.D. at Columbia).

Eight years after my first visit, I had the opportunity to make another trip accompanied by my wife. This time I did not spend all of my time mainly in one place. On this trip, which was sponsored jointly by the State Department and National Science Foundation, I visited the twelve most active and generally prestigious psychological laboratories. They included (from soth to north) Kyushu, Hiroshima, Kwansei Gakuin, Nagoya, Osaka, Tokyo, Tokyo University of Education, Waseda, Keio, Tohoku, and Hokkaido. About seven weeks were spent in visiting the laboratories and lecturing. The visits to the laboratories involved talking to the research people and instructional staffs, and taking extensive notes on the experiments, personnel, and laboratory equipment. Sometimes this program took part of a day and sometimes two or three days, particularly when I was called on to lecture or conduct a seminar. Usually, after visiting each laboratory I would return to my hotel and dictate my notes. The tapes were sent back to Columbia and I finally completed a report in November after my return to New York in late September.

My experiences at Professor Motokawa's physiology laboratory is one I remember well. Shortly after arriving by plane from Tokyo, my wife and I went with Professor Motokawa to his laboratory and viewed some experiments for about half an hour. Thereafter, totally unprepared, I faced about twenty graduate students. When Professor Motokawa introduced me and said, "Now Professor Grahan will discuss our work with us, "I was more than a little surprised. In one way or another, we participated in some sort of discussion, the Japanese in a sometimes imperfect English, and I in terms of an imperfectly understood experimental context. At any rate, we were able to discuss certain matters of vision relating to the experiments I had seen, and we completed the discussion without serious misunderstanding, or so it seemed to me. Of course, in this and similar experiences in other laboratories, the success of the visits depended on members who spoke English,many very well and others only understandably.

OFFICE OF NAVAL RESEARCH, LONDON

The academic year,1952-53 was my first sabbatical year. Although I had been officially a member of several academic staffs after 1930, the fact remains that sabbaticals had, except in

possibly one or two universities, ceased after the Depression and during World War II. They were renewed after the war and my turn came in 1952. Before I left for Japan in August, I had made arrangements to spend my leave working as a scientific liaison officer with the Office of Naval Research, London, and my wife and I went there in October, 1952.

The duties of a scientific liaison officer involved visits to psychology and related laboratories in various countries of Europe, and writing informative accounts of the work being done in the laboratories, the people doing the work, and a generally descript - ive version of the backgrounds of research. These reports were subsequently distributed to American colleagues and other interested or officially designated persons. The liaison officer, through his visits to laboratories and associated activities, such as giving a paper on some topic of interest to members of a particular laboratory, provided an important source of scientific inter-communication.

An important part of this activity involved the distribution of reprints and reports from American laboratories. The liaison officers in the life sciences (Biology, Medicine, Psychology) had little contact with classified research (I don't remember that I ever reported or transmitted any classified material), but the representatives of other sciences, mainly Physics, did deal to some extent with classified documents.

The contacts between the liaison officer and his European colleagues resulted in many friendly relationships. I believe the overall effects of the interchanges that took place were useful to all the scientists involved. For me, the experience was personally satisfying. I visited about twenty-five laboratories and discussed many matters. I also attended a number of professional meetings. I met many active and well-known scientists, including major figures in psychology as well as in related fields. I remember some val uable discussions, particularly with my old friend Granit in Stockholm at the Karoline Institute, and with Professor Toraldo at the Institute of Optics in Florence. Having some months before reviewed work on Mach bands, I discussed this topic with Professor Toraldo, whose work in the area has since been outstanding. His interest in it developed because as a physicist he first thought of it as a peculiarly recalcitrant topic in diffraction optics.

A high light of my experience in Europe was my visit to Professor Michotte's laboratory in Louvain. I spent a total of three days there viewing the experiments on causality and discuss-ing psychology with Professors Knops, Nuttin, and Montpellier as well as Michotte. I was particularly happy to see Professor Mont-pellier again, for we had spent months together in the early '30's when he was a post-doctoral fellow working with Hunter and I was a young assistant professor.

A BOOK: VISION AND VISUAL PERCEPTION

In 1948 I had a number of conversations with representatives of Wiley and Sons, including most importantly Professor Langfeld of Princeton, who was advisory editor for psychology. We discussed the possibility of my writing a book on vision and, although I knew that the project would be long and arduous, I finally agreed to do it. It was soon obvious that the book was more than should be undertaken by one man, and I therefore decided to interest a group of former students and colleagues to contribute various chapters to a large volume of which I should be editor and an author. If we had foreseen the problems and difficulties which were to face us until the publication of the volume, Vision and Visual Perception, in 1965, I suspect that we would have been even more hesitant to write it than we were in the beginning. The co-authors were John Lott Brown, Neil R. Bartlett, Yun Hsia, Conrad G. Mueller, Lorrin A. Riggs, and myself. The book was generally well received by many reviewers, especially by some in Europe.

AN APA PROJECT

As a member of the APA Policy and Planning Board that in 1952 formulated a plan that developed as Projects A and B, I served on the steering committee. Project A was concerned with psychology as a scientific discipline and was directed by Sigmund Koch, then of Duke University. Project B was concerned with problems of personnel and their training; its director was Kenneth E. Clark, then of the University of Minnesota. My contribution was mainly to Project A, and then as a contributor to the Koch volumes. I saw a great deal of Koch in London in 1952-53 while he was a Fulbright Fellow at University College. I wrote a rather long manuscript on color vision, its data, and systematic nature for Volume 1 (1958) and, with Philburn Ratoosh, an article for Volume 4 (1962). The latter article contained some aspects of discussions from other theoretical papers of mine plus contributions made by Ratoosh and some new material of my own. The article on color vision in Volume 1 may be regarded as a first version of chapters in Vision and Visual Perception; in fact, in some details it is more exhaustive.

FINAL THOUGHTS

My main work in psychology has been concerned with research and the training of graduate students. Except for a short interval devoted to the running behavior of the white rat and some wartime experiences with selection and training procedures, my investigations have involved problems of vision and visual perception. Of nearly equal importance from my point of view has been my interest in theoretical and systematic problems, particularly the formulation of psychophysical and perceptual behavior in an objective psychology.

These endeavors have been honored by my election to the National Academy of Sciences, the American Philosophical Society, the American Academy of Arts and Sciences, and by my being awarded the Warren Medal of the Society of Experimental Psychologists, the Tillyer Medal of the Optical Society of America, the Distinguished Scientific Contribution Award of the American Psychological Association, the President's Certificate of Merit, and a Guggenheim Fellowship.

It is surely true that the area which has claimed my attention has not been the most popular in psychology, but it has, nevertheless, for me been a most intellectually stimulating and enticing one. It has no doubt satisfied needs which have dominated my development as a scientist. I am happy that my activities have allowed me to participate not only in the nominal area of psychology, but also in related scientific areas such as physiology. For a certain kind of person a feeling of enhanced scientific scope is additionally rewarding. However, I am glad to report that after taking the Strong Vocational Interest Inventory three times over a period of thirty-three years (in 1935, 1948, and 1968) my highest score with great reliability was in psychology. I like to think that I may be forgiven if I take this finding as a happy confirmation of my choice of profession.

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